Second Report of the Winchester College Natural History Society. Second and Third Years. (Winchester: J. Wells, 1873.)

THIS Report contains a record of the doings of the Society from May 1871 to February 1873, and is thoroughly encouraging, and certainly a great contrast to the Reports of the two other public schools recently noticed in these pages. The Winchester Report proves that, by judicious management, a School Natural History Society may be made to yield most gratifying results.

The present Report for the two last years, although its record of the earlier papers is incomplete, shows that the Society has been fully alive, and has been growing quietly and steadily, and doing real and satisfactory work. The numbers of the Society are not at this time actually full. But it appears that the elder half of the members are nearly all of them real workers, and it is hoped that the younger half are learning to be the same. It is of more consequence, as the Preface rightly says, that those who belong to the Society should be working members, than that its numbers should be swelled by names. The meetings of the Society have been well attended, and there has been apparent an increasing appreciation of the opportunities afforded by the meetings for showing and seeing objects of interest, as well as for reading and hearing papers.

It is satisfactory to see that old members take an interest in the society after leaving school, several of them contributing valuable papers.

In general, however, we are extremely glad to see, the papers have been those of actual members, and the Society may well feel satisfaction at the increasing readiness, ability, and completeness shown by the leading members in supplying papers at its meetings. The papers which have been read by the Secretaries, Hall, Goddard, and Forbes, may, perhaps, the Preface says, and we think justly, be specially remarked, as combining ability with knowledge based upon personal observation. It is in these papers that the growth of the Society's work has been chiefly seen, and in which its main value consists.

The collections belonging to the School have been considerably increased. The cabinets attached to the Moberly Library now contain about 4,000 specimens, and more are waiting to be mounted and added.

Among the "desiderata" the Preface mentions the following, in case old Wykehamists, or other friends of the School, may be able and willing to supply them:—In Entomology, specimens of Notodontidæ and Pyralides, amongst Lepidoptera; and of any other orders than Lepidoptera. In Conchology, recent Brachiopoda, and Pteropoda. In Geology, Fossils from any of the Primary Formations, Wealden Beds, Red Crag, and Coralline Crag.

The Report contains a number of very interesting papers, mostly by Messrs. E. H. Goddard, W. A. Forbes, and C. S. Rayner, evidently three of the most industrious members of the Society: all the papers are evidence of original observation and independent thought on the part of the writers. The first-mentioned contributes the following papers:—"Hymenoptera," "Botany and Entomology" (in which the localities in the district are indicated in which the collector will reap the best harvest of flowers and butterflies), and one on "Gall Insects." Mr. W. A. Forbes contributes papers on "Coleoptera," "British Reptiles," and "Mimicry and Protective Resemblance." Mr. Raynor contributes a useful paper "On the Different Methods of obtaining Lepidoptera," and a very careful and interesting one "On the Different Modes of Concealment and Defence practised by Insects." The Report also contains a paper on "The Diamond Fields of South Africa," sent by Mr. E. A. Hall. Appended are very full and carefully compiled Botanical, Entomological, and Geological Lists. We hope the next Report will contain a list of the local Fauna, which it is proposed to form.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Perception and Instinct in the Lower Animals

I HAVE waited some time in the expectation that some of your readers would have asked Mr. Wallace a very obvious question with regard to the incident he adduces of a dog finding his master five months after having been lost, and in a house which the latter "had not contemplated going to or even seen before the loss of the dog." (NATURE, vol. viii. p. 66.) In seeking to account for this thoroughly authentic and highly remarkable case, Mr. Wallace observes: "Could it have obtained information from other dogs . . . . ? Could the odour of persons and furniture linger two months in the streets? These are almost the only conceivable sources of information; for the most thorough-going advocates for a "sense of direction" will hardly maintain that it could enable a dog to go straight to his master, wherever he might happen to be." Now, there is yet a third supposition open to us, and it is one which, in the absence of information, is certainly the most probable. Can Mr. Wallace's friend remember whether he had been walking in the vicinity of his new house during the day upon which the dog returned? *i.e.* can he be sure the dog did not trace his footsteps? That a keen-scented terrier is able to distinguish and to follow his master's track in a public thoroughfare, however densely it may be crowded, I know from the success of searching experiments.

With regard to dogs communicating information to one another, I may mention that I have often observed them doing so. According to my experience, the dogs must be much above the average in intelligence, and the gesture they invariably employ is a contact of heads with a motion between a rub and a butt. It is quite different from anything that occurs in play, and is always followed by some definite course of action. I must add, however, that although the information thus conveyed is always definite, I have never known a case in which it was complex—anything like asking or telling the way being, I believe, quite out of the question; so far, at least, as this action is concerned. One example will suffice. A Skye terrier (not quite pure) was asleep in the room where I was, while his son lay upon a wall which separates the lawn from the high road. The young dog, when alone, would never attack a strange one, but was a keen fighter when in company with his father. Upon the present occasion a large mongrel passed along the road, and, shortly afterwards, the old dog awoke and went sleepily down stairs. When he arrived upon the door-step his son ran up to him and made the sign just described. His whole manner immediately altered to that of high animation, and, clearing the wall together, the two animals ran down the road as terriers only can when pursuing an enemy. I watched them for a mile and a half, within which distance their speed never abated, although the object of their pursuit had not, from the first, been in sight.

As the instinct question seems to have come to a close it is desirable to observe that the only outcome of its discussion has been to intensify the previous belief in the existence of some unexplained faculty, which may be provisionally termed a sense of direction. Mr. Wallace, in his general reply, avowedly ignores all those cases adduced by your correspondents in which his theory cannot possibly apply; *e.g.* dogs describing the third side of a triangle, or returning by land whence they had been taken by sea. He says: "Several of the writers argue as if I had maintained that in all cases dogs, &c. find their way, wholly or mainly, by smell; whereas I strictly limited it to the case in which their other senses could not be used" (vol. viii. p. 65). Now, whether or not Mr. Wallace originally intended his letter to raise the general issue as to the presence in dogs of a sense of direction, this has certainly been its effect, so that the instances he here refers to are not in any way beside the question which immediately arose. I have much too high an esteem for Mr. Wallace to say anything that might lead to a discussion with him, but it is evident that these remarks have no such tendency; for, if he admits, as he candidly does in the sentence just quoted, that his theory cannot apply to all cases, it necessarily follows that, even could he prove it to be true in some, the fact, although of considerable psychological interest, would leave the question as to a sense of direction just where it was before.

It should be borne in mind that dogs are not the only animals

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in which this sense appears to be present. It is popularly believed to occur in members of at least two orders of Insects, viz. white ants and bees, but I am not aware that any authentic cases have been recorded. Horses and cats seem to possess it in a high degree, and sheep must either have wonderful memories, or owe their return, in numerous cases, to the faculty in question. Still more wonderful, if we deny them this faculty, must be the memory of migratory birds, some of which return, after months of absence and over thousands of miles, to the same nests in successive seasons. If we allow them this faculty it is not, from analogy, improbable that migratory mammals and even fishes are likewise endowed with it. The most conspicuous example, however, is perhaps that afforded by carrier pigeons. To take one case: two or three years ago some of these birds were flown from the Crystal Palace to Brussels, and it stands, if I remember correctly, upon the authority of Mr. Tegetmeier, that they arrived within a few minutes of a telegram despatched from the Palace at the moment they were liberated. Now, in this case, even the extravagant supposition sometimes made that carrier pigeons are guided by the sight of their destination is excluded, for, as these birds are not high-flyers, the curvature of the earth between London and Brussels would prevent them from seeing the latter. And, even if we imagine that these particular pigeons occasionally towered to obviate this difficulty, yet the curvature of the intervening clouds would have imposed another quite as effectual.

There is still one important point which has not been noticed during the discussion of this subject. We possess indications that this sense of direction, like other mental capacities, admits of cultivation by exercise, and, indeed, that it may remain altogether latent and useless until thus developed. If these indications represent generalities we have at once an adequate explanation of the apparently capricious manner in which this faculty occurs.* As this communication is already too long, I shall here be brief.

It is, I believe, a recognised doctrine among fanciers that carrier pigeons, however purely bred, must be educated by flying short distances before they can be depended upon for long ones. I remember having myself lost a valuable bird by flying him, for the first time, at a distance of 500 yards from his nest. Although in full view of it he became utterly confused, taking long flights in various directions, and ultimately went straight out to sea.

Here is an analogous case in a mammal:—I kept a terrier, of highly intelligent parentage, enclosed in a yard with high walls from the time of its birth until it was eighteen months old, and then took it out for the first time, along the sea-shore. The experiment elicited several facts of psychological interest, and one of them has bearing upon the present subject. Part of the coast over which we went and returned was rough with large shingle, and the terrier's locomotive power being very limited, it was unable, on the homeward journey, to keep up with my pace. Desiring to see what it would do if left alone, I continued for half a mile, and waited to see it come up. As it did not do so, I returned, and found that the animal had actually reversed its direction and gone fully a quarter of a mile from the place where I had left it. After having been taken out short distances seven or eight times, it was inadvertently lost in a neighbouring wood. Now, it had only been in the wood once before, yet its appreciation of direction had made so great an advance that it returned an hour afterwards. As this terrier never evinced any disposition to track footsteps, I do not think its return was due to scent. Anyhow, in a few weeks it became an inveterate wanderer, roaming over the country far and wide. GEORGE J. ROMANES

Dunskaith, Ross-shire, July 7

Comte on the Survival of the Fittest

MR. JEVONS called attention some time ago to the desirability of preparing a list of past thinkers and writers who have held, in strength or weakness, the doctrines of Darwin and Spencer. Mr. Darwin has himself named a few of those authors, and Prof. Haeckel has extended the number. Recent communications in NATURE show that the list is as yet incomplete. In reading Comte's "Cours de Philosophie Positive" a few years ago, I was impressed with the general similarity of certain doctrines therein stated with some of Darwin's theories. Referring recently to the 42nd lesson of that course (t. iii.)—"Considerations générales sur la philosophie biotaxique," I find that Comte, in reviewing the Lamarck-Cuvier controversy, says:—

"Toute la célèbre argumentation de Lamarck reposait finalement sur la combinaison générale de ces deux principes incontestable, mais jusqu'ici trop mal circonscrits: 1°, l'aptitude essentielle d'un organisme quelconque, et surtout d'un organisme animal, à se modifier conformément aux circonstances extérieures où il est placé, et qui sollicitent l'exercise predominant de tel organe spécial, correspondant à telle faculte devenue plus nécessaire; 2°, la tendance, non moins certaine, á fixer dans les races, par la seule transmission héréditaire, les modifications d'abord directes et individuelles, de manière à les augmenter graduellement á chaque génération nouvelle, si l'action du milieu ambiant persévère identiquement. On conçoit sans peine, en effet, que, si cette double propriété pouvait être admise d'une manière rigoureusement indéfinie, tous les organismes pourraient être envisagés comme ayant être, à la longue, successivement produits les uns par les autres, du moins en disposant de la nature, de l'intensité, et de la durée des influences extérieures avec cette prodigalite illimitée qui en coûtant aucun effort à la naïve imagination de Lamarck." (1st ed. "Cours de Philosophie Positive," t. iii. pp. 560 and 561.)

Modification and heredity are here strongly asserted, and the conditions of unlimited transformation as strongly sketched. In continuance of the same argument, Comte, on p. 563, objects to Lamarck's hypothesis, of which he thought very highly as a logical effort:—

"Qu'il repose, ce me semble, sur une notion profondément erronée de la nature générale de l'organisme vivant. Sans doute, chaque organisme determiné est en relation nécessaire avec une système egalement déterminé de circonstances extérieures, comme je l'ai établi dans la quarantième leçon. Mais il n'en résulte nullement que la première de ces deux forces co-relatives ait dû être produite par la seconde, pas plus qu'elle n'a pu la produire: il s'agit seulement d'un équilibre mutuel entre deux puissances hétérogènes et indépendantes. Si l'on concoit que tous les organismes possibles soient successivement places, pendant un temps convenable, dans tous les milieux imaginables, la plupart de ces organismes finiront, de toute nécessite, par disparaître, pour ne laisser subsister, que ceux qui pouvaient satisfaire aux lois générales de cet équilibre fondemental; *c'est probablement d'après une suite d'éliminations analogues que l'harmonie biologique a dû s'établir peu à peu sur notre planète, où nous la voyons encore, en effet, se modifier sans cesse d'une manière semblable.* Or, la notion d'un tel équilibre général deviendrait inintelligible et même contradictoire, si l'organisme était supposé modifiable a l'infini sous l'influence suprême du milieu ambiant, sans avoir aucune impulsion propre et indestructible."

The struggle for existence and the survival of the fittest or natural selection are here acknowledged. What is more, the fact that the eliminations due to unfitness for the environment or medium have produced and is producing biological harmony, is pointed out. I have not met with any passages outside of the writings of the new school, which are more explicit than these, though it must not be understood that their author was a transformationist. The preface to the volume in which this occurs is dated "Paris, le 24 Février, 1838." In his general table appended to the sixth volume of his work, Comte says that the Leçon from which these extracts are taken was written between the 9th and 15th of August, 1836. J. D. BELL

New York

The Glacial Period

CAN you inform me if anyone has suggested the following explanation of the existence of the glacial period? And is the explanation I am about to offer a possible one? I put the question in all diffidence, for I have not carefully studied the theory of heat: you must therefore regard any utterance of mine on the subject as merely "a random arrow from the brain." Well, then, it seems to me that the quantity of heat given out in a unit of time from a unit of surface of an intensely heated globe, such as the sun, does not follow the law of radiation of bodies moderately heated. What I mean is this:—It is quite possible that at a time when the sun's mean temperature was higher than it is now, his rate of radiation might have been less; the quantity of heat emitted by him in a unit of time less than it is now. For since his chromosphere must have been thicker, and his solid or fluid nucleus somewhat less in diameter, I suppose that the radiation of the nucleus must have been more retarded by the

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* In connection with these points compare the suggestive remarks of Mr. Darwin, contained in the two concluding paragraphs of his article on Instinct (NATURE, vol. vii. p. 418).

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